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CASE OF HYDROPHOBIA.

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THE following account was principally written from data furnished by Mr. J. Stearns, Jr., Surgical House-pupil of the Mass: General Hospital, who took much interest in investigating the facts of the case. The patient was a male child, by name Patrick Murphy, 3¼ years of age, living at 69 Endicott Street, and was brought into the Hospital on June 25th, 1859.

Exactly five weeks before, he was bitten by a dog six or eight months old. The animal at the time was not thought to be rabid, although on the same day he had "snapped at and slightly bitten" a man in the hand, as was thought at the time from playfulness not unusual with puppies. The little boy had, at the time, a cracker in his hand, which the dog attempted to seize, taking into his mouth with it the whole of the right hand, and inflicting a wound on each side of the wrist. The wound on the anterior surface was from the half to two thirds of an inch in length; that on the opposite side was like the mark from a simple puncture. The wounds at the time were treated by Dr. Owens, who cauterized them not long after the injury, and ordered a poultice. There was no further treatment of them. They were very sore for a time, particularly the one in front, but the child continued as well as usual in his general health, and nothing remarkable occurred till a week previous to his admission.

At this time the mother's attention was drawn to the patient by what she called a "dulness" coming over him, followed by a "silliness and listlessness." Four days before his entrance to the Hospital was the first onset of the paroxysms, which were described by the mother as having been quite formidable; they were very violent when water was brought into his neighborhood, so that the mother was obliged to give up washing the child. He manifested a desire to take food and drink from his mother, though on attempting to swallow he was quite unable to effect it. For this reason he took scarcely any nourishment for four days

before he was brought into the Hospital. The preceding facts were principally obtained from the parents of the child.

On his entrance into the House, he was in a highly excited condition, tossing his head, and throwing about his limbs in every direction. He spit violently, or attempted to do so, as if his mouth was full of feathers, or tow; occasionally crying out, or snapping at those about him, saying that he wished to bite them, and they must get out of his way. His eyes were very bright, his face pale, and there was a lividity about the eyelids and generally over the whole surface, with a quivering of the lips and muscles of the face, and constant tremor of the whole body. On taking a dose of morphine he was quieted, and the nurse prevailed on him to swallow some milk from a mug. After a time he drank a whole mug full, and ate a small piece of cake. His manner of taking the milk was not as if he had any aversion to it, but from apparent consciousness of the effort necessary to swallow. He clutched violently at the mug, with eyeballs starting out, and the whole frame undergoing the greatest agitation. The effort of swallowing was attended with a sense of suffocation, and the corners of the mouth were strongly retracted. He exhibited the same symptoms on taking cake; and from his great desire for both, appeared to be suffering much from hunger. A viscid discharge was observed to take place from the mouth, by the nurse and others. The urine was passed in great abundance through the afternoon and evening.

He became quite calm through the great attentions of the nurse, who seemed to inspire him with confidence, and went to bed with him in her arms, in spite of the remonstrances of those about her. He talked incessantly and incoherently, though at times he could be understood. He seemed to appreciate much the kindness of the nurse, and told her he should bite her; but when she put out her arm to him, he kissed and stroked it with his hand. He had several paroxysms after his entrance, with intervals of comparative quiet, the attacks being only of short duration, lasting about five minutes each. In this condition he continued most of the night, with constant watchfulness and tossing about, and at half past three, A.M., he died in one of the convulsive attacks.

No examination of the body could be obtained from the parents.

Mr. Stearns, at my request, visited the house at which the child had resided, for the purpose of obtaining some more facts in regard to the case, but did not elicit anything of importance beyond the preceding. He saw the wife of the man who was bitten on the same day with the little boy; the bite was a very slight one, on the joint of one finger, and the woman said no blood came from it. The man promised to be at the Hospital on the following day for me to examine it, but for some reason did not appear. The dog was drowned, and Mr. S. could get no further history of it. A superstition existed with them, of which they informed him, that

if the dog could have been killed by one of the family, the patient would have escaped; also, that if the liver of the dog could have been applied to the wound, the effect would have been equally efficient, which of course naturally implied the death of the dog.

In connection with this case of hydrophobia, I would remark, that about twelve or fifteen years since, I proposed, at a meeting of this Society, for the purpose of obtaining information, the question, whether any case of this affection had ever occurred in Boston, or whether there was any tradition of one in the New England States; but no answer was elicited in the affirmative.

The first case reported in Boston appears to be that of Dr. Coale, in October, 1848, which was followed shortly afterward by that of Dr. Curtis, in Lowell, supposed to have been caused by the same dog, which had escaped from Boston and made his way to the latter city. This was followed by other cases in various directions, running through a course of two or three years, during which time I saw in consultation, in Brookline, a patient of Dr. Wild, and the case of a child brought into the Hospital within twelve hours after having been bitten, where the parts were freely cauterized at the time, and within twenty-four hours from the time of the accident cut out by Dr. Cabot. This patient returned home within four weeks apparently perfectly well, but by the expiration of another week the disease appeared, and she was returned to the Hospital with all the symptoms similar to those detailed above.

All these patients died after three or four days' illness, the attack coming on in an average of about five weeks from the reception of the injury.

Since that period the contagion, if it may be so called, or inoculation, seems to have exhausted itself, and but few cases have been recorded until lately, when rumors have begun to be heard of its re-appearance. I have constantly had persons call to consult me with very severe bites from dogs, but not finding from them that the animals had shown any signs of rabies, I have not thought it warrantable to apply so severe a remedy as cauterization, or excision, to an accident so common. Under the present circumstances, i. e., a disposition to rabies among the canine race, I should feel myself called upon to make a thorough application of the nitrate of silver to the wound, as recommended by Mr. Youatt, who considered this remedy as almost infallible if applied immediately, and who from his liability to be bitten always carried a bit of caustic in his pocket, and had many times made use of it with effect on his own person. Or, if circumstances required, free excision should be made of the injured part.

The following remarks of Mr. Youatt are of so much value that I have extracted them at some length: "The wound should be thoroughly washed and cleansed as soon as possible after the bite

is inflicted; no sucking of the parts, as is advised by many, for the purpose of extracting the poison, as the presence of a small abrasion of the lips or interior of the mouth would most assuredly subject the parts to inoculation. If the wound be ragged, the edges may be taken off with a pair of sharp scissors; the wound must then be thoroughly cauterized with nitrate of silver (lunar caustic), being sure to introduce the caustic into the very depths of the wound, so that it will reach every particle of poison that may have insinuated itself into the flesh. If the wound is too small to admit of the stick of caustic, it may be enlarged by the knife, taking care, however, not to carry the poison into the fresh cut, which can be avoided by wiping the knife at each incision. Should the wound be made on any of the limbs, a bandage may be placed around it during the application of these remedies, the more effectually to prevent the absorption of the veins. Nitrate of silver is a most powerful neutralizer of specific poisons, and the affected parts will soon come away with the slough, no dressings being necessary, except perhaps olive oil, if there should be much inflammation of the parts. If the above plan be pursued, the patient need be under no apprehension as to the result, but make his mind perfectly easy on the point."

A question has been frequently asked, whether these symptoms might not be of a tetanic character from the irritation of the wound. There has not been the slightest appearance of trismus, or locked jaw, in any of the cases I have seen, and the lapse of time from the reception of the wound has been too long to be attributed to such a cause, the wounds having healed, and for the most part having shown little signs of irritation.

At the moment of writing this article, I have had a case of trismus or locked jaw at the Hospital, which, although not severe, affords an opportunity of comparing this rare disease with hydrophobia. The patient was a woman, 45 years of age, in pretty good health, who had a large plank fall upon her, producing a compound fracture and dislocation of the ankle-joint. I saw her about half an hour after the reception of the injury; the lower extremity of the tibia projected through a large wound at the ankle-joint, the internal malleolus being broken off and left in the wound. This I removed with a knife, so as to allow me to restore the dislocated bone to its proper place, with the hope, in the first view of the case, to save the limb. On further examination, however, when the restoration of the bone allowed of a more full investigation of the joint, I found the injury of the tibia to be complicated with a comminuted fracture of the fibula, some bits of which lay loose in the joint. Another fracture of the fibula also existed about half way up the limb. Amputation of the leg was therefore resorted to by the double flap, just above the upper fracture, in what appeared to be sound parts.

Although everything seemed to be favorable for union by the

first intention, yet the wound partially suppurated, and put on a sloughy appearance, the vital powers of the tissues having probably been injured by the blow, although this at the time was not apparent. The patient, however, complained of little or no pain, but seemed to be quite comfortable and in good spirits, though with little appetite; she had no fever, and no other symptoms of constitutional irritation.

On June 30th, when I visited her in the morning, she told me that her jaws were stiff, and she could only open them about a quarter of an inch by taking hold of them with her hands. She said that she had felt some soreness in her jaws for about four days, but had not thought it of sufficient importance to mention. I at once suspected the nature of the disease, and requested Mr. Stearns to keep a close watch upon her, and inform me if anything unusual occurred; I also encouraged her to take something of a stimulating nature. She herself was not advised of our suspicions.

In the afternoon she was suddenly taken with a slight tetanic spasm, great difficulty of breathing, and coldness of the extremities; stimulants were administered, hot applications made to the feet, with other external remedies, and when I saw her about 6, P.M., she was in a very comfortable condition. Her jaws at this time had to be pried open with a bit of stick. I ordered a drachm of the solution of the sulphate of morphia to be administered every three or four hours, and as much brandy to be given as she was disposed to take.

She passed a very quiet night under the treatment directed, and on the following day pronounced herself much relieved; the stump was suppurating freely, and gave her no pain.

The mental condition of this patient and of the one with hydrophobia, it will be perceived, were strikingly different. This one was perfectly calm and collected, unlike the semi-delirious, agitated and violent state of the patient with the latter disease, the pulse not much affected, being rather below than above the natural standard; in hydrophobia it is very rapid, as has been the fact in all cases of locked jaw that I have seen. To a person who has seen the two diseases, I do not think it would be very easy to make a mistake in the diagnosis.

THE EFFECTS OF THE CONSANGUINITY OF PARENTS UPON THE MENTAL CONSTITUTION OF THE OFFSPRING.

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PERHAPS no opinion, upon subjects of a medical character, is more widely diffused among the public, or more tenaciously held, than that the results of the marriage of blood relations are almost uni-

formly unfortunate. This opinion has been so long held and so often reiterated, that by sheer force of these circumstances alone it has come to be regarded as an unquestioned and unquestionable fact. Almost every one, from the forward school-boy and the wise old woman, to the dignified and erudite divine, will repeat that the common experience of all time and every country has proved it so conclusively, that to doubt or question its accuracy is tantamount to an exhibition of either the grossest ignorance, or the most wilful and culpable obstinacy, or a weak desire of appearing wiser than one's generation by the enunciation of an astounding paradox.

But notwithstanding this opinion is so general, it is by no means clear that it is so ancient as is commonly supposed, or that the ancient laws forbidding the marriage of relations were founded upon an experience of the evil effects of such alliances. It is evident that the laws of the Jews, in regard to this subject, were founded upon a different idea, for in the same place (Leviticus) where the marriage of blood relations is forbidden, we find that alliances between those who are closely connected, but still not *related*, are also forbidden, in almost the same terms. Indeed, throughout the whole of these commands, by the language employed, as well as the fact related above, it is evident that the grounds of the prohibition were merely the preservation of public decency between the sexes, among those who, from family connection, would naturally be thrown together upon terms of great intimacy and friendship. The fact that the sons of Aaron were commanded to marry their own sisters, and ever afterward to marry their own kindred, may be taken as another proof.

It is often said that this opinion is the result of a long-continued observation of the evil effects of such alliances. But it seems to me highly probable that it had its origin in times much more recent than this would imply—most likely within a few centuries. These laws have been regarded as divine commands, and a mistaken harshness of piety has apparently fixed upon a deterioration of the offspring as a fitting punishment for their infringement.

However the idea may have had its origin, it is certain that it is strongly fixed in the public mind, and that previous to a very recent period, it was completely destitute of anything like proof of truth. In fact, it could not, by any courtesy, have been considered anything more than a wide-spread and vigorously-maintained prejudice. But a prejudice of this kind * does not remain entirely with the non-medical public. It invades the profession, and accordingly this one was nearly as strongly fixed and as destitute of foundation with them as among others. If the opinion had always remained in this crude state, even among physicians, it might well

* It is a curious and remarkable fact, that nearly all the most whimsical, and at the same time widely-spread popular superstitions (and also those most firmly fixed), connected with physiology and pathology, have reference to generation and the sexual system.

have been left to itself; at any rate, it would have been unworthy of a serious and labored refutation. But of late years it has become altogether more respectable, through the fact that several bodies of statistics have been collected by different physicians, to determine its truth, and that these, in a greater or less degree, have appeared to sustain the popular views of the matter.

But although it appears to have been agreed upon that the sins of the parents are (or ought to be) visited upon the children, in their possessing a less perfect organization than others, it does not seem to have been clearly determined exactly in what manner this inferiority shows itself. It has been averred that conception is less likely to occur, and that when it does occur, it comes to an end by abortion. (In Leviticus, among the punishments denounced against those breaking some of the laws of marriage, is the childlessness of the parties committing the offence.) Next, various imperfections in vigor of constitution, resulting in premature death, also in development, in a greater or less degree, from hypospadia and hare-lip to the most strongly-marked monstrosities. Finally—and in our own country this is the most widely entertained of all—mental hebetude of various degrees, even to complete idiocy, and a peculiar predisposition to the various forms of insanity. These are certainly formidable results, as stated, and deserve a most careful and unprejudiced investigation, and, if true, a reprobation of such alliances. But are they true? Medical statistics are almost proverbially apt to be deceptive, and I believe that those collected for the elucidation of such points as these—already firmly rooted in popular belief—are *peculiarly* apt to be so. It seems to me that there are good grounds for the opinion that those which have been brought before the public for the determination of the question are unworthy of being depended upon with that firm reliance which figures are always supposed to command. This is not one of the questions, to the determination of which, it is intrinsically difficult to apply the numerical method. On the contrary, it seems to be one to which it is peculiarly applicable. It is capable of being answered with absolute certainty in this way, if the statistics gathered only represent with accuracy the real state of facts.

The chief reason why I consider them unreliable, is, that a variety of circumstances concur to bring those instances in which the results of such alliances have been unfortunate, prominently forward, so that they alone are taken to represent the results of the totality of such marriages. Before glancing at the reasons which induce me to this belief, a brief statement of the conclusions arrived at by those authors who have adduced such statistics, will contribute to the elucidation of the subject.

The first writer who has taken this method of investigation, so far as I am aware, is Devay *(Hygiène des Familles)*. His work was originally published in 1846; since then, the number of his

cases has been considerably increased. His deductions are founded on 121 instances of the marriage of relatives. These are reported in rather a loose manner. The fact upon which he lays most stress, is the great number of these unions that were sterile: of these, there were 22. Then come the abortions, of which there were 17. There were also 17 cases of malformation of the hands by excess of parts, and 2 cases in which there was deficiency. There were also several other cases of malformation of greater or less degree, from that of an anencephalous fœtus to that of a patch of white hair upon the crown of the head. Among these, was 1 case of spina bifida, 2 cases of harelip, and 1 of congenital ichthyosis. The only instances in which there was any disorder of the nervous system, are one case of deaf-mutism and one of epilepsy. *There was not a single instance of idiocy or mental weakness or insanity.* The author has omitted to mention the number of children resulting from these marriages: we cannot reasonably suppose that they were less than 400, and it is probable that they were considerably more.

Dr. Rilliet, of Geneva, has also engaged in investigations on the subject. His conclusions are similar to those of Devay, but I am unable to give the data upon which they are founded. But the most able and elaborate article yet published, is that by Dr. Bemiss, in the Transactions of the American Medical Association for 1858. He reports a great variety of observations; the following is an abstract of the most important results:—Total number of cases, 833; number of children, 3942; number defective, 1134; deaf and dumb, 145; blind, 85; idiotic, 308; insane, 38; epileptic, 60; scrofulous, 300; deformed, 98; sterile, 53.

These abundant statistics may be thought sufficient to set the question forever at rest. Dr. Bemiss remarks, that they "are a sufficient number to warrant the belief that any additions thereto, *if procured in the same manner*, would not materially affect the ultimate result." This is probably true. These two series of statistics appear to have been collected essentially by the same means; viz., the report by various physicians of those cases which they happened to be conversant with, or which they themselves collected by other means (probably from the reports of non-professional persons, or those cases which common rumor had rendered notorious). Dr. Bemiss has not overlooked the fact that his cases have a degree of uncertainty about them, for he remarks that "it is natural for contributors to overlook many of the more fortunate results of family intermarriage, and furnish those followed by defective offspring or sterility."

In determining a medical question of this kind by means of statistics, peculiar care ought always to be exercised in presenting a fair average of cases. If this point is neglected, the result, whatever it may be, cannot be regarded as possessing more than a greater or less degree of probability. And when, as in these

cases, it is impossible, from the number of contributors and our ignorance as to their characters, to determine how much care has been exercised, we are totally at a loss to determine how much credit we ought to accord to the truthfulness of the result. In these cases, it appears to me, from a variety of considerations, that it ought to be very slight.

The multitude of influences that concur to render the statistics obtained in this manner unworthy of reliance, may be exhibited by comparison with a kindred subject. Suppose that we wished to determine, by statistics, the effect upon the offspring of an ungratified desire for strawberries, or other similar articles, on the part of the mother during pregnancy. Who can doubt but that in a majority of the cases we should be able to collect, by any method similar to that which has been employed in determining the effects of consanguinity, we should find that the offspring were "marked"? Or if we collected all the cases we could obtain, where women had been frightened by an owl, that the majority of children would have heads resembling that bird? And yet it is difficult to see why the statistics of these authors, obtained in a similar manner, upon a similar subject, which is connected with an equally powerful and wide spread, but vague prejudice, should obtain more credit than would be accorded to the above supposed cases. Even if these latter cases were obtained from physicians exclusively, though they would be more apt to be correct than otherwise, still the same causes of error would exist to an extent sufficient to cause them to be regarded justly as a mere burlesque upon medical statistics. All physicians are not philosophers, neither are all exempt from popular prejudices. I am not aware that they are less apt to be influenced by a previously conceived prejudice than other persons; and many of them are prejudiced in regard to this matter. These would be just the ones who would be likely to reply to circulars like those of Dr. Bemiss, as they have a theory to sustain. Statistics collected by such, would be little likely to present a fair average of cases. Besides, in those collected in the manner already spoken of, we have no guarantee that they were all personally known to the reporters; it is highly probable that many of them were collected from vague rumor alone.

How a preconceived prejudice blinds the minds of those possessed by it, is exhibited in the following extract from M. Devay. He had conceived the opinion that enchondroma was one of the effects of these marriages upon children, and the following is his method of substantiating it. "A young woman affected with phthisis entered the Hotel Dieu. Besides the principal disease, she had upon the left hand three tumors (enchondromata). Several times while she lived, she was interrogated concerning her family; we insisted upon knowing whether her parents were not in some degree or other related. She replied that she did not know, but that such a thing might well be, seeing that in the country where

she was born, families very often intermarried within themselves." If cases are to be collected in this way, there is no theory which cannot be substantiated, and the statistical method may be regarded as inapplicable to medicine, upon better grounds than those upon which so many are disposed to object to it. How can we suppose that subordinates would be more accurate than the principal in collecting cases? When cases gathered by a single individual of known reputation are so little deserving of credit, what ought we to think of them when a multitude of hands have been employed upon them, all working perhaps upon different principles?

It seems to the writer, from these considerations, that independent of the results arrived at, the statistics of M. Devay and Dr. Bemiss deserve but little credit. I believe this suspicion will become a certainty by a comparative examination of the tables presented by these two gentlemen.

In France it is the popular opinion that marriages of relatives are sterile in a greater proportion that common, while idiocy, insanity and deaf-mutism are not much thought of in this connection. In this country, on the contrary, these latter are believed to follow such alliances with great regularity, while sterility, or a less degree of fertility, are not esteemed one of their common results. Here no one hears the remark made of such marriages, that they have no children: it is always, "their children are all idiots." Now if the popular prejudice peculiar to each of these countries has been allowed to intrude itself into these two series of statistics, we ought to see its effects upon these two supposed consequences of these alliances. And we *do* see them, as the following comparison will show.

Devay—Total No. of Marriages,	121;	Sterile,	22.
Bemiss— " "	833;	"	53.

Quite a number of the cases of sterility in the American table, had been married but a few years. Still, in spite of this, the proportion is nearly three times as great in the former as in the latter. As there is no prejudice in this country that such marriages are likely to be sterile, so we find that the statistics obtained here do not give a greater proportion of such than probably exists in society at large. (Of 140 cases of marriage among people not related, and who lived in one neighborhood, there were 12 instances of sterility—a proportion considerably greater than the above cases show.) It is also commonly believed, in France, that these unions are not only more apt to be sterile, but that those not so have fewer children than usual. In this country there is no such prejudice, that I am aware of. The average number of children, as given by Dr. B., in those unions which had ceased producing (including those sterile), was 5.88 to each marriage. In the cases he gives for comparison, of persons not related, it was

6.70 for each couple. This latter is certainly considerably above the average. In the report to the Ohio Legislature, however, the number of children among those related was actually greater than among those not related.

As being the converse of this, let us compare the same tables, as to idiocy, insanity, deaf-mutism and epilepsy:

	No. of Children.	Idiots.	Insane.	Epileptic.	Deaf-Mute.
DEVAY—	572*	0	0	1	1
BEMISS—	3942	308	38	60	145

Here, again, we see the same results—where there is no prejudice existing, we find no greater proportion of unfortunate cases than we should expect to find in society at large. On the contrary, in the cases of Dr. B., they seem enormously large. I believe the personal knowledge of cases, by almost every one, is sufficient to convince him that by no means so great a proportion as 1 in 13 of the children of relatives is an idiot, even if we go no farther than first cousins.

I do not know that there is a prejudice here as to epilepsy occurring often among the children of these unions; but the fact that epilepsy and dementia (the distinction between dementia and idiocy is commonly overlooked, even in the profession) so often stand in the relation of cause and effect, is sufficient to account for the large number of cases of this disease in the table.

Again, though not a popular prejudice, it is theorized that the children of these marriages are more apt to die young than others. We should certainly expect this to be the case, if what is so often repeated in regard to them is true, viz., that the vital force is less in them than in others. Of the 3942 children of such marriages, in Dr. B.'s table, 883 "died young." This is rather an indefinite expression, and of itself shows that the original reporters exercised little care or accuracy. If we regard the expression "young" to be limited to those under five years of age, the proportion of those "dying young" in the table is considerably less than what occurs in society at large. Ordinarily, about 30 per cent. of those born die before arriving at the age of five years, while here only about 22 per cent. are mentioned as dying young. If instead of five years, we take ten as being "young," the difference would appear still more strongly in favor of the children of kindred. If we are to trust the conclusions arrived at as to insanity, idiocy, &c., we certainly ought to conclude that the children of relatives possess a more vigorous constitution than others. But instead of either of these conclusions, or both of them, being true, it would appear more reasonable to doubt the accuracy of the facts upon which they are grounded. In the cases given by Dr. B. of persons not related, as a standard of comparison, only about 16 per cent. of the children are reported as "dying young."

* Estimated, bearing the same ratio to the marriages as in Dr. B.'s table.

Abundant statistics, gathered without any specific object being had in view in their collection, show that this is too small a proportion. The same is true in regard to the "scrofulous." Who can doubt that there is some mistake, when among 837 children of persons not related, only one is set down as being a subject of that dyscracy? If such evident errors as these are to be found here, where there is no special reason for inaccuracy, we might with reason look for still greater ones in that part where prejudice comes in to assist carelessness.

These considerations seem to me abundantly sufficient to show that these statistics—all that have ever, as yet, been given to the public—should be regarded only as exhibiting the strength of the hold that this singular prejudice has upon the public mind, and the difficulty that is experienced and always will be experienced in the collection of accurate statistics, in the face of such a wide-spread theory. How few there are, who would not be willing to stretch a point, in order to furnish an argument against an alliance so repugnant to all our ideas of public morals as that of brother and sister would be.

In collecting cases to illustrate this subject, it is perhaps entirely impossible to do it without non-professional assistance. The only way by which we can arrive at the truth by statistics, with anything like an approach to accuracy, is by going over a given population, and obtaining absolutely every case of the marriage of relatives among them; and even then, in spite of our strongest efforts, I believe the popular prejudice would still creep in, and mar the result of our best endeavors. Indeed, it seems to me that this is one of those questions (by no means rare in medicine) to which statistics are almost inapplicable.

To illustrate the subject, however, as far as possible, I have collected *all* the cases of marriages of relatives in a small town, of somewhat less than six hundred inhabitants. It was taken without any previous knowledge of what the result would be. The following table will exhibit the number of these cases, and the number and condition of the offspring.

No. of Cases.		No. of Children.	Deformed.	Dead.	
1	First Cousins.	3	0	1	
2		12	0	5	
3		6	0	3	
4		2	0	0	
5		5	2 (hare lip.)	3	Of the deaths, 5 were occasioned by scrofulous diseases. Only 10 of them died before the age of 5 years; of these, 2 were stillborn.
6		4	0	3	
7		2	0	1	
8		3	0	1	
9		7	0	3	
10	Second Cousins.	2	0	1	
11		4	0	0	
12		3	0	1	
12		53	2	22	

There was not a single case of idiocy or insanity among them. The two cases of malformation (hare lip) might appear to sustain the common prejudice; but, upon examination, only prove that this defect is often hereditary, for it had occurred frequently in the family for several generations. Undoubtedly, the intermarriage of relatives would tend to strengthen any such hereditary tendency. There was no case of barrenness among them, and the number of children, though apparently small, was greater than in the average of marriages in the same town among those not related. For among 120 marriages, in which the parties were not related, there were only 416 children. The small number of children is accounted for by the fact that many of the parties had not ceased having children. The number of deaths among the offspring of those related, is also quite as small as is ordinarily the case. The number of deaths from scrofulous diseases, and also the number dying under five years of age, would compare favorably with those whose parents were not related. There were no cases of deafness or blindness among them.

One word as to the number of these marriages of consanguinity in the community. Dr. Bemiss concludes that there are about 5000 in the United States. I believe this number is far too small. In the place referred to above, no account was taken except of first and second cousins, yet there was one such marriage to every fifty persons. It is probable that in the small towns of New England such marriages are more common than elsewhere; still, making every allowance for this source of error, it seems to me that their number cannot be less than ten times as great as Dr. B. supposes. This supposition would allow only one such marriage to every 500 or more inhabitants.

The statistics I have given are complete as far as they go, and they are gathered in the only way in which we can place any dependence upon their accuracy, that is, by taking every case in a given population. Still, they are imperfect, for in some neighborhoods families are so mingled by intermarriage that it is difficult to tell what their exact relationship is. There are also obviously numerous other sources of error in the determination of so complicated a question by means of statistics. In this state of things, the assistance of reason and analogy will be quite as likely to give us the truth as a collection of suspicious facts; at any rate, they ought not to be neglected, as being unworthy to be trusted.

Among people of education, and those given to speculation and theory, there is the same opinion in regard to the results of breeding "*in and in*" among animals, that obtains in regard to the human race, among all. With those, on the contrary, who are practically engaged in stock raising, there is no such opinion—at least in this country. Among the latter, with animals possessing desirable qualities, it is the universal custom to breed in and in, in order to preserve or strengthen these peculiarities. And this is done without

any fear of deterioration in other respects. We often hear it remarked, that if no evil results show themselves in the first generation from the marriage of kindred, yet they are sure to occur if the same process is repeated, and this to such a degree that to escape is the rare exception. It is common to see breeding "in and in" carried to an extent among animals that is extremely rare in the human race. A familiar instance of this is seen in that breed of horses known as the *Morgan horses*. Breeding from kindred has been so common among them, that some of them still have three fourths or more of the blood of the original Morgan horse, and this although there have been as many as half a dozen generations intervening. Yet far from there being anything like degeneration apparent, these animals are universally celebrated for vigor of constitution, endurance and long life. The same course has been pursued in regard to the English hunting and racing horses; the result has been, that the descendants surpass their ancestors in all those qualities which indicate a high degree of perfection of physical organization. There is probably no country where this system has been so long pursued as in England; and there is no country which can compare with it in its horses.

It has been stated, too, that fowls, when bred in and in, degenerate, and that this is especially exhibited by a kind of hermaphrodism, in which the plumage of the cock and hen approximate to each other and the power of propagation is entirely lost after a few generations. Those who are familiar with the customary mode of managing these fowls, will at once recognize the untruthfulness of this account, so far as it relates to this country. On farms there is usually but one cock kept, so that in a few years, through the rapidity of their breeding, the whole flock must necessarily be nearly homogeneous in blood. Such results as those alluded to above, must be exceedingly rare, if they ever occur, so that they may, probably, with truth, be attributed to other causes.

If we commence at the lowest grade of life in which there is any appearance of sexual organs, we shall find that they almost universally exhibit a high degree of productiveness, and that likewise they are propagated from what we may regard as the highest degree of relationship—both sexual organs being placed upon the same individual. This is manifest throughout by far the largest number of plants, and is almost universal in the lowest grades of animal life. How can we suppose, if there is any general law in nature, causing a diminished power of reproduction in those individuals most nearly resembling each other, that these most striking examples would escape its influence?

A little higher in the scale of animal life, we find a phenomenon showing itself in the lowest of the genuine bi-sexual animals, which shows most conclusively that when nature has fixed the two sexes upon different individuals, she still has no antipathy to reproduction from kindred. This is the phenomenon of *parthogene-*

sis, or the impregnation, by the male, of several successive generations at once—a method of reproduction exhibiting itself in some of the most prolific of the forms of animal life. Here the last of the series of generations is as numerous, perfect and prolific in their turn, as the original. It is well known that bees, ants, and various other insects, propagate continually, as it were, by a sort of incest, if we may use the term, a single queen producing a swarm which does not separate for the purpose of accouplement, but continually reproduces within itself. Here we not only see no natural antipathy (as is so often repeated regarding the most closely resembling individuals of the higher animals) between individuals of the same race and blood, but, on the contrary, a strong instinctive propensity to continue the race by the union of kindred.

Still higher in the scale, among the birds and mammalia, we not only find no signs of this sexual antipathy among kindred, but that instinct of safety which keeps a flock or herd together while they are young, still exercises its influence, in a less degree, to be sure, but yet with sufficient strength to render it far more probable than not that they will breed together subsequently.

The same instinct has its influence upon savage nations, which always have a less proportion of idiots and deformed than the civilized, where marriages within close degrees of relationship are rare.

The theory of the imperfect offspring of parents closely resembling each other in physical constitution, is discountenanced by various other facts. We know that animals of species widely separated—that is, of a most diverse physical organization—are unable to produce a hybrid offspring. As they approach each other more nearly in form, we find them beginning to manifest the sexual passion for each other, and capable of producing young; but these young themselves are incapable of continuing the hybrid variety. As they approach still closer in physical structure, and we arrive at varieties of what were originally a single species, this latter peculiarity has disappeared. Offspring are produced, themselves capable of continuing the crossed variety, but still neither so perfect nor so prolific as the parents. We see this phenomenon most clearly in the human race, in those varieties most widely separated—viz., the white and black races. It is a fact which has long been recognized that mulattoes are neither so perfect in physical organization, so active mentally, nor so prolific, as the parents on either side. There is certainly more antipathy existing between these races than between individuals of the same race, however closely resembling each other. The same is true of races less differing in appearance. It would certainly be a singular anomaly if the series should stop just at this point, and we should find that a certain degree of difference was most favorable to the reproduction of perfect organisms, and as we recede from this point, in

either direction, the offspring were less perfect. It would certainly be against the analogy of all nature; and until more trustworthy statistics than have yet been presented, prove it to be the case, we may well doubt whether the marriage of relatives is one of the causes which fill our insane and idiot asylums with abortive and diseased minds, and our general hospitals with the wrecks of physical frames.

CARBONATE OF AMMONIA IN MEASLES.

[Communicated for the Boston Medical and Surgical Journal.]

MESSRS. EDITORS,—Having seen the carbonate of ammonia used in scarlatina with very satisfactory results, I was induced, about two years ago, to try its effects in measles. Since that time, I have used it extensively in that disease, and with most satisfactory results. For about two months past, this disease has prevailed quite extensively in this vicinity, and in a very severe form. In nearly every case that I have been called upon to attend, I have prescribed the carbonate of ammonia, and in every case where this has been given, the disease has come to a speedy and favorable termination. The medicine should always be given early in the disease, before the eruption appears; or if not, immediately upon its making its appearance.

My usual prescription is: R. Ammoniæ carb., ʒi.; aquæ camph., ʒiss. M. Dose, a teaspoonful three times a day, varying the dose according to the age of the patient and other circumstances.

Cedar Falls, Iowa. S. N. PIERCE, M.D.

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON, JULY 14, 1859.

THE EPIDEMIC OF SMALLPOX.

THE prevalence of smallpox in Boston for several months past, to a very unusual extent, is a fact that ought to attract public attention, and to call forth an expression of opinion as to the means whereby such an epidemic may be prevented in future. Until a year ago, the disease had been almost unknown in Boston for several years. In each of the months of March, April and August, 1858, there was but a single fatal case; whereas, since the beginning of the present year, there have been 32 deaths from this disease. Moreover, the epidemic seems to be on the increase. The first death occurred in February. In March there was 1; in April there were 5; in May, 7; and in June, 18.

It is feared that there are many parents who neglect to have their children vaccinated, notwithstanding every facility that is offered. The City Physician's office is open daily to all who wish to be pro-

tected against smallpox, without money and without price, and the dispensary physicians are always ready to vaccinate those who apply to them. Moreover, there are certain physicians who make a good profit by exporting vaccine lymph to distant parts of the country, and who will almost pay for the privilege of vaccinating for the sake of securing the virus. Yet the number of those who, through ignorance, stupidity or prejudice, neglect to avail themselves of these opportunities, is, we fear, large. If to the many unprotected children in the city, we add a considerable number of adults in a similar condition, who arrive daily from the country by railroads and steamboats, we shall see that there is material enough to furnish a good supply of cases.

We know not whether it would be possible to extend vaccination farther, by law. In England, vaccination is compulsory; but though a good deal of benefit might be done in some districts by a law making it obligatory, in this State, yet (supposing it possible to carry it into effect) we should still be at the mercy of unprotected strangers, who arrive among us daily.

Until the blessings of registration are more clearly known and appreciated, we cannot hope for much improvement in the state of public vaccination. In proportion as the causes of disease are laid bare, and the comparative ease with which they may be prevented made evident, will be the desire to apply the remedy. These 31 deaths during the past few months might every one have been prevented, had the subjects been vaccinated: but this mortality represents but a small part of the evil. Every death from smallpox represents a number, varying from 4 to 10, according to circumstances, of cases which recover, and every case of smallpox represents a much larger number of cases of varioloid. We are told that upward of 200 cases of the latter disease occurred in a single ward of this city, during the winter and spring. Nearly, if not quite all, this amount of sickness might have been prevented by the adoption of means, not difficult and expensive, but most simple and easy of application. If vaccination were thoroughly carried out, smallpox and varioloid would practically cease to exist.

We should not omit to state that the City Physician has recently, without solicitation, sent vaccine lymph to every physician in Boston, and it is well known that he is always ready to furnish it to such medical men of the city as apply for it.

Our object in making these remarks is not so much to suggest any special means of extending vaccination, as to call the attention of the profession to the subject, in the hope that some feasible plan may be proposed by others, for which purpose we respectfully request those of our brethren who are interested in the matter to favor us with their views.

UNCHANGEABLE SOLUTION OF PROTOXIDE OF IRON.

Messrs. Editors,—A preparation of iron, bearing this name, was made and introduced to the medical profession some months ago by Nichols & Co., Chemists, Boston. The mode of its preparation was described at the outset in a circular, freely issued to practitioners and druggists. I have used it in my practice with great satisfaction, either in its pure state, or in combination with syrup of ginger, whiskey, or

iodide of potassium. It is now in extensive use, and is found to be an agreeable and efficient form of iron.

Recently, however, a liquid bearing the same name, but differing in taste and color, has been thrown into the market by Charles T. Carney, druggist, of your city. This article differs so much in appearance from the *one originally* introduced by the *other* manufacturers, that I was induced to submit it to an examination. I observed that Nichols's Protoxide of Iron colored the cork of the bottle containing it; Carney's did not. Carney's effervesced on adding carbonate of soda; Nichols's did not. On adding aqua ammoniæ to each, Nichols's, on shaking, instantly turned to a dark color, shortly precipitating a large amount of protoxide of iron: Carney's manifested no change of color and gave no precipitate, so that a more careful and analytical examination of Carney's seemed to be necessary. This examination proved Carney's article to be a very small amount of iron in the form of a citrate, with a large excess of free citric acid. The amount of iron in the specimen procured at his counter is less than *one fourth* of that stated by him upon the label of the bottle in which it was obtained, namely, twenty grains to the ounce.

Finally, then, Carney's Protoxide of Iron contains *no free protoxide of iron*, but does contain 4.09 grains to the ounce of the *citrate of iron*, with an excess of citric acid. CHARLES H. ALLEN.

Cambridgeport, June 17, 1859.

HOMŒOPATHY IN MASSACHUSETTS.

MESSRS. EDITORS,—Permit me, as a member of the Massachusetts Medical Society, to offer a cordial concurrence in the sentiments expressed by you with regard to Homœopathy, in the article termed "Homœopathy in Connecticut and Massachusetts," published in the Boston Medical and Surgical Journal of June 23d.

Your opinions, so ably advocated, are in fact precisely those of nearly every member of the Massachusetts Medical Society: then why should they not avow them, as you have done, and by proclaiming these opinions in a collective form, compel Homœopathy to roll upon its own pellets, and sink or swim in its own dilutions?

For what purpose was the Massachusetts Medical Society instituted, and what was the primary motive of its revered founders? Was it not to protect the public against the specious encroachments of empiricism, and to place the medical profession as far as practicable upon an elevated and honorable platform? If so, does it not seem that individual self-respect and collective dignity, as well as obligation to the public, imperiously demand that the Massachusetts Medical Society should adopt a resolution stamping Homœopathy (in *their* opinion) as *unmitigated quackery?* Then we should at least have the satisfaction of performing our duty, and the public cannot in future charge us with indifference to their welfare, or ridicule us as too cowardly to define our position. Very respectfully yours, C. M. W.

The unusual specimen of description of the symptoms of disease which we printed in our issue of June 9th (p. 386), and which might well have evoked the angry shade of Lindley Murray himself, has elicited from a highly respected correspondent, the following example of a similar nature.

"*——*——, *June 10th*, 1859.

"MESSRS. EDITORS,—The case on page 386 of your JOURNAL, this week, reminds me of one that came to my notice a few years ago, for chemical examination for poison.

"The man suspecting that a Boston chemist, who examined it and reported '*no poison*,' had been bribed, brought it to me, and when I reported '*no poison*,' he told me of the Boston examination, and went away satisfied.

"I think, in description of symptoms, this writer is fully up to yours, and if from his therapeutics you can tell to what school he belongs—all well.

"Perhaps the record may amuse some of your visitors, as it has not a few of mine. Truly yours, *——."

"The first colt—taken Dec. 25 on tuseday and died Dec. the 26 on Wennesday—the next colt was taken Dec. the 29 & Died January the 3 Day on thirsday—the next Colt was taken January the 3 Day on Thirsday and Died January the 5 Day on Sataday—as killed [i. e. was killed, intentionally, by his owner]—Give Linsit Oyl and hens Agues —Flaxseed tea—Green tea—Peperment tea—One was bled the First thing—Blud Fomed & frothed beyond Reason—the Food hardsgrass hay and Read top—they lay inund [in under] a warm shed—Not used A tall—Wauter handy—ben yoused for years—it is good wauter—they began to stager a little—it incresed—Got down in 24 hours—they had great thirst—they breat smelt bad—No miss-stake after they got down—they eys reed and inflamed—Great heet—swelled bad round eys—and all over No misstake they had fits—Last Neara Minnit without breething they swet bad—they stumic when taken out the inside Darke brown sum blud bludy wauter—about half the stumuck was good and called helty.

"I had Great Docters and Fairyers and them had seen poysoned horses—they all give in—Poyson surposed to be Arsnic Arsenuck—the horse Grate his teath—lays his Nose back of his fore Lage, Grunt soe you cood hear them as Much as ten Rods—they Pawed all the time when not in fits. G. I. D . . . . . ."

Our correspondent, in a postscript to his note accompanying the above remarkable production, suggests an explanation of the term "oapen" of the head, which occurred in the sample of English composition published by us, June 9th, and which we were puzzled to interpret; he says:—"the 'oapen' of yours is like the 'inund' of this [communication]—'oapen' may be *uppen* or upper part of the head, as 'inund' is *in under*."

We regard the explanation as a very ingenious one—indeed, quite a philological triumph. After all, these two specimens of English are not worse, considering their sources, than many manuscripts we have received from persons who attach the title of Medicinæ Doctor to their names. Were we to print some of these *as they are sent*, the effects would, we are persuaded, be wonderful, varied and peculiar.

Ohio State Medical Society.—The 14th Annual Meeting of the Ohio State Medical Society was held at Columbus on the 7th, 8th and 9th of June. The usual routine of business was gone through with, papers on the subject of Epilepsy, Insanity, Cataract, Mercurials, Surgery, and Obstetrics were read; and a public lecture on Drunkenness was delivered by Dr. Wright, of which 3000 copies were ordered to be printed for general distribution. But one member of the Society,

it was stated, had died during the last year. The following complimentary vote, relating to a distinguished member of the profession, and personally well known in this part of the country, was adopted:

"Inasmuch as Prof. R. D. Mussey, from its origin a member of this Society, is, on account of age and infirmity, now resident in a distant city, and disqualified for active duty in the profession; therefore,

"*Resolved*, That in respect to his distinguished character, we request him to allow the Society to abate his assessments and continue his membership, and that the Secretary is hereby instructed to inform Prof. M. of this action."

Measures were adopted to secure a medical department in the State Library.

Newspaper Puffs by Correspondents.—There are two kinds of puffs—the puff quack direct, and the puff quack procured, and we have been asked which is the most disreputable. We do not hesitate to say that the puff quack procured is decidedly the more dishonest of the two. The quack that goes boldly into the newspaper, horn in hand, and proclaims his own shame, like a highway robber, risks the consequences, and is entitled at least to the reputation of a villainous gallantry. But the skulking empiric, who does this by proxy, and would gain the spoil while he preserves his bacon—would hold with the hare and run with the hounds—adds a cowardice to his cupidity that sinks him immeasurably beneath the contempt of the contemptible. We would feel a less oppressive sense of degradation in the recognition of the one than of the other, and we would "starve and rot" before we would recognize either. We are sorry to say that these puffs are generally to be found in religious papers—so called. One dear brother writes to the dear brother editor, of the wonderful success of dearly beloved brother, Dr. so-and-so.—*Nashville Journal of Medicine and Surgery.*

THE New British Pharmacopœia is being prepared by the Medical Council. At present there exist three works of the kind, under directions respectively of the College of Physicians of London, Edinburgh, and Dublin, producing a confusing diversity in pharmaceutical practice. The object is now to reconcile these inconvenient discrepancies, and produce a uniform national pharmacopœia.

Much temporary annoyance, it is believed, will be produced by the change, but the result will be satisfactory, and will create a uniformity which has been much needed.

It has been decided to adopt the Avoirdupois weight instead of the Troy weight, or Apothecaries' weight, and the work will be published in the English language instead of the Latin.—*Peninsular and Independent Medical Journal.*

Dr. J. S. SULLIVAN has withdrawn from the *Savannah Journal of Medicine*, as Senior Editor. Dr. Juriah Harriss is now editor, and Dr. R. D. Arnold Associate Editor.

THE publication of the *Medical Gazette* of Louisville, and the *Medical Chronicle* of Montreal, has been suspended.

Health of the City.—Among the interesting facts relating to the last week's mortality is the great predominance of the deaths of females over those of males, being no less than 16. Six of the deaths from consumption, and 4 of those from smallpox, were those of females, as were all who died from scarlatina and from cancer. We notice 5 deaths from pneumonia, 4 from "puerperal diseases," and 1 from sunstroke. The total number of deaths during the corresponding week of 1858 was 40, of which 11 were from consumption, 2 from pneumonia, 1 from scarlatina, 1 from puerperal disease, 1 from sunstroke.

Books and Pamphlets Received.—Lectures on the Diseases of Women. By Charles West, M.D. (From the Publishers.)—A History of the Discovery of the Circulation of the Blood. By P. Flourens. Translated from the French by J. C. Reene, M.D. (From the Publishers.)—A Lecture on Idiocy. By John M. Galt, M.D.—Cleveland Medical Gazette, a new journal published monthly at Cleveland, Ohio.

DIED,—In Milledgeville, Geo., Tomlinson Fort, M.D., an old and distinguished physician of that place.

Deaths in Boston for the week ending Saturday noon, July 9th, 66. Males, 25—Females, 41.—Accident, 5—apoplexy, 1—inflammation of the brain, 1—cancer, 3—consumption, 7—cholera morbus, 1—croup, 1—dysentery, 3—dropsy, 2—drowned, 1—infantile diseases, 4—puerperal, 4—epilepsy, 1—scarlet fever, 4—gangrene of lungs, 1—gastritis, 1—disease of the heart, 2—hæmorrhage of the lungs, 1—intemperance, 1—inflammation of the lungs, 4—marasmus, 1—measles, 1—old age, 1—premature birth, 1—smallpox, 5—sunstroke, 1—syphilis, 1—teething, 3—tumor, 1—unknown, 1—whooping cough, 2.

Under 5 years, 26—between 5 and 20 years, 4—between 20 and 40 years, 21—between 40 and 60 years, 11—above 60 years, 4. Born in the United States, 43—Ireland, 17—other places, 6.